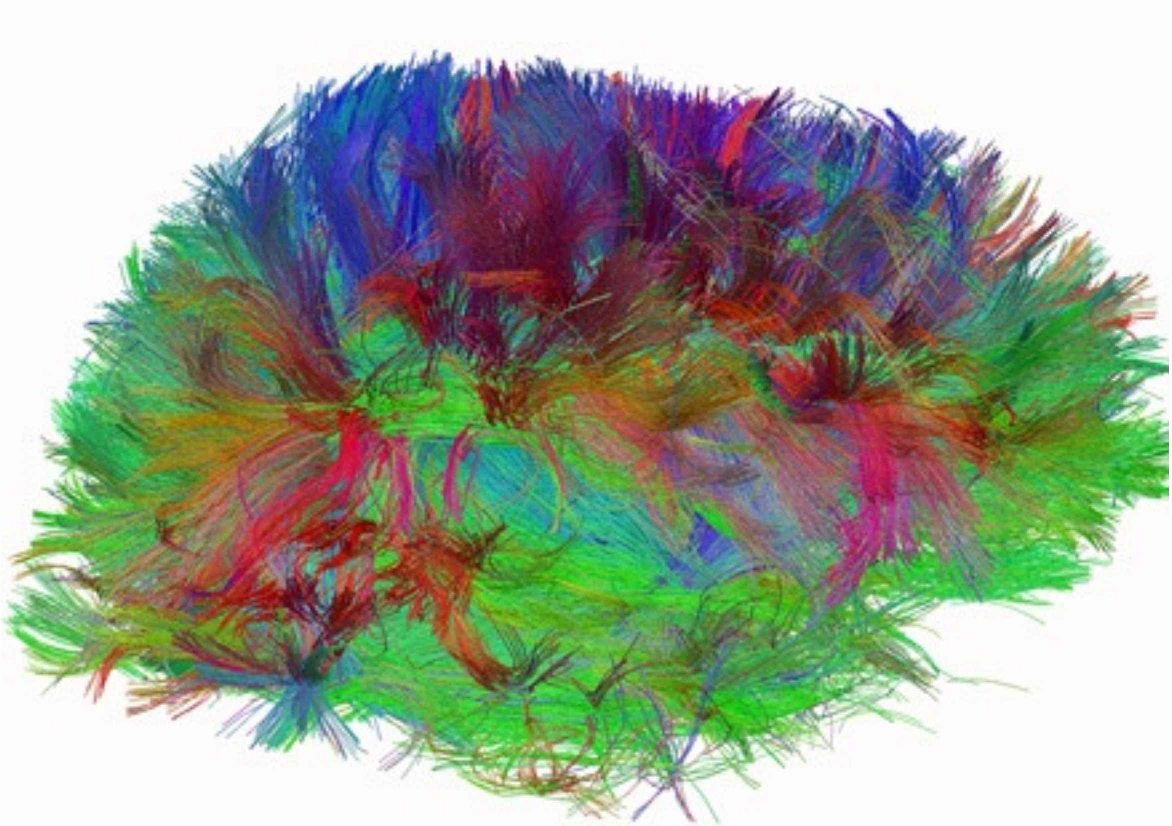
The Potential of a (nearly) Complete Structural Neuroanatomical Model of the Human Brain



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Introduction

"The real obstacle to reduction, it is claimed, is the intentionality of mental states; that is, the fact that they have content. For explanations at the intentional level are of a radically different nature from those at levels that do not advert to representations and content. The latter explanations have a good and useful place, but they cannot do justice to the representational dimension of psychological life."[1]

Generally, the main skeptical opposition that projects like the Connectome face are statements like: It is all very well to explain the machinery and mechanisms of the brain, but what will that really teach us about our selves, our minds, what makes us really tick, our mental life? People want to know, "well, what is this really going to teach us?" This kind of skepticism is grounded in a basic preference for the familiarity of the theories of folk psychology. There is nothing entirely wrong with having that preference for your average layman, but this is a dangerous idea if it is used to guide the study both of the mind and the brain. I propose that part of the very compelling reason that a project like The Connectome will be revolutionary to our understanding is precisely because it's object of study is not the mind, nor the brain. Its object is the mind-brain. This is a uniquely important step for cognitive sciences because it embodies the proper object of study: the mind-brain. To be sure, the explananda of mind-brain will evolve over time as greater understanding of the object is gained, but this initial recognition of mind-brain as the proper object of psychological and neuroscientific knowledge is a crucial one on the path of gaining understanding.

In this paper I set out to argue, and I hope to convince you, that the proper object of study for the cognitive sciences is the mind-brain. This is not merely a misunderstanding of the structure-function distinction as many might say, and it is not an effort to demystify or quantify every experience or deny experience that every human being could or will have. But, rather, a framework, for pursuing study of both mental and brain phenomena while discovering both mental and brain mechanisms. For, the case I want to make is that brain and mind are not distinct, as some functionalists would have it. I argue that in seeking to know the hows and whys of mind, we will inevitably learn about and need to know the brain, and in seeking to know what the brain does and how it functions, we need to know the mind and how it functions. The two are inextricably linked and any philosophy of mind, of psychology, of cognitive science needs to acknowledge the mutual role both the study of the mind and the brain has for gaining knowledge.

Further, I believe that it is erroneous to suggest that one could gain knowledge of one without the other: for the two are two lenses on the same subject. It is for this reason that I argue that a project with as far-reaching aims as The Connectome Project, has deep understanding and implications of what the philosophical problem for both the study of the mind and the brain is. The Connectome Project, far from being merely the latest and greatest in a series of empirical forays into the unknown "for the fun of it" have real bearing on how we will conceive of psychological and neuroscientific problems in the future. In fact, we do not yet know how far knowledge of the connectome will stretch into scientific disciplines and it is conceivable that a project such as this will have implications for genetic research, evolutionary theory of human minds, language, learning, memory, all aspects of neuropsychology, etc. To ignore its efforts, its triumphs and its failures would be to impede real progress in cognitive scientific knowledge, and, at its worse, it would mean relegating the study of psychology and its objects into a historical study not unlike mythology.

I have three goals for this essay:

1) To introduce the reader to the NIH's Connectome Project, its methods, aims, and discoveries.

2) To set this project against a background of materialism/physicalism that supports a minimal functional view, but not the ontological commitments of some kinds of Functionalism(s).

3) To lay out the benefits of such a philosophical view for making predictions, and generating hypotheses.

Chapter One: What Would A Connectome Be?

Connectome Project: methods, aims, progress

A major project, supported by the NIH (National Institutes of Health) Harvard-MGH/UCLA and WU-Minn Consortiums, is underway on the frontier of human mind/brain relationship connections research. The Human Connectome Project, so-named to recall the scope, far-reaching effects, and efforts of the human genome project, is an attempt to map and record every neuron in the human brain and its connections to each other. I contend that in order to use this project's information fruitfully, we must understand its contribution to psychological and cognitive science explanation as a part of the material reductionist project. Explanation must make reductionist contact with functional types in order for this project to make any sense. Upon arriving at the Human Connectome page, one is greeted by the following message:

> Navigate the brain in a way that was never before possible; fly through major brain pathways, compare essential circuits, zoom into a region to explore the cells that comprise it, and the functions that depend on it.The Human Connectome Project aims to provide an unparalleled compilation of neural data, an interface to graphically navigate this data and the opportunity to achieve never before realized conclusions about the living human brain.

A wide and far-reaching project is opened before us and the promise of its findings should fill us with energetic optimism about the future of mind/brain research; but first we must dispel with Cartesian dualist intuitions and its legacy of philosophical inheritances. Ramón y Cajal had always spoken of the importance of having an accurate basic anatomical map of the brain itself prior to the investigation of its physiological and psychological functions.[2] Francis Crick has often spoken about the basic necessity for hypothesis formulation and testing of an anatomical map of the brain, at a level where functional characteristics can be mapped on successfully.[3] A network analysis of the brain lends itself to comparison to organizational brain theories such as Fodorian Modularity and Massive Modularity. Such an analysis could be used to test organizational hypotheses and organizational-dependent functional hypotheses. A fine-grain detail anatomical map of the brain would be a useful comparison tool for neuropsychological deficits, genetic influence studies,[4] structural-functional relationship studies, evolutionary comparison studies, and therapeutic effect studies. It could be a baseline comparison against which to measure variance in the population and between conditions and in development. So, there are lots of motivations, but I want to focus on findings that suggest that dynamic, complex-system self-organization principles (in development) contribute towards structure-functional relationships found in the human brain.

The Connectome Project(s) have been greatly influenced by work done in the *C. Elegans,* a roundworm with 302 neurons, and work with a larger nervous system in the *Aplysia californica,* a sea slug with 18,000 neurons. The totality of neuronal connections have been anatomically mapped for the *C.Elegans* and such a scientific achievement has inspired much research on nervous system organization and function in other species. However, nervous system organization has significant differences in vertebrates and species that exhibit dimorphism (sexual difference). So, study of the nervous systems of vertebrates and particularly species evolutionarily related to humans (such as macaques) remains of high neuroscientific interest. The Connectome Project(s) is an effort to understand principles of nervous system organization in humans so as to illuminate aspects of functional organization, which may have important implications for functional classes of mental phenomena and behavior in humans. That is the hope, anyway.

The Connectome Project(s) seek to detail, as much as possible, structural and functional brain connections of subjects under a variety of brain imaging techniques: fMRI, EEG, DTI. MEG, and MEA. These "combinations of physiological and anatomical techniques have allowed patterns in neuronal connections to be identified, leading to the identification of neuronal microcircuits and the formulation of probabilistic connection rules."[5] Small-world networks (discussed in section Network Motifs, Self-Organization Principles, Graph Theory) have been found to be ubiquitous in the brain and in naturally-occurring systems. These identifications of microcircuits have been found to be highly positively correlated across structural and functional analyses. DTI (Diffusion tensor imaging) has shown that key structural features of small-world network connectivity. "Current evidence suggests that topological parameters [of small world connectivity such a hub centrality measures, path length and clustering] are generally conserved between structural and functional networks."[6]

Such measures of small-world networks, which are a class of scale-free networks, are a result of complexity. "Complexity arises in the macroscopic behavior of a system of interacting elements that combines statistical randomness with regularity…and[7] is shaped by interactions among their constituent elements."[8] Such complexity found in the human brain echoes existing knowledge that we have of development and network elaboration which compromises between the competing demands of developmental genetic imperatives and environmental (cellular, regional, global, external) statistical unpredictability. Such competing inputs to the system can be conjectured to be contributing to the apparent organization of the system. However, this remains to be explored in detail.

Questions about the relationship between structure and function will continue to haunt empirical efforts such as The Connectome Project, however, there are some initially encouraging results about the possibility of reductive contact being made between these two classes of knowledge. Some plasticity studies of mammalian cellular networks suggest that these are constantly undergoing change, whereas other evidence indicates that most synaptic spines are stable.[9] The relative stability of structural networks provides a point of reference in studying brain networks. Using structural networks as a comparison for studying functional networks, there has been much encouraging evidence that suggest initial avenues to pursue the structural-functional relationship questions. "Patterns of functional connectivity undergo spontaneous fluctuations and are highly responsive to perturbations, on a timescale of hundreds of milliseconds. These rapid reconfigurations do not affect the stability of global topological characteristics. On longer timescales of seconds to minutes, correlations between spontaneous fluctuations in brain activity form functional networks that are particularly robust…[these] comprise the default mode network."[10] These 'resting state' (there is some disagreement about this measure)[11] patterns provide an opportunity for comparison against structural networks and initial findings "suggest that structural connections are highly predictive of functional connections."[12] This is an early and rough suggestion about how structural and functional networks share topological connectivity features. Obviously, there is much more to be explored by way of how structural features support flexible functional connectivity on the variable timescales that exist.

Some early explorations into how structure and function might relate use computer simulations, with empirically derived parameters of connectivity to see what kinds of activity such structural relationships might support. Such studies have "demonstrated the emergence of complex spatiotemporal structure in neural correlations at multiple timescales."[13]

There are many features of small-world networks (again, see Network Motifs, Self-Organization Principles, Graph Theory for discussion) such as clustering, reciprocal connectivity, and hubs (or modules) that have been found in synaptic connectivity circuits.[14] Several areas of the brain have been found to act as structural and functional hubs such as the precuneus, the insula, and the superior parietal cortex and the superior frontal cortex.[15] Such findings of network similarities between functional and structural networks present the intriguing hypothesis that these functional aspects can be explained on the basis of its central position in the network.[16] There are also initial indictions that structural motifs confer specific functional abilities upon a network.[17]

Models as Instruments & Mediators of Knowledge

Modeling the human brain requires an immense amount of data. The human brain has an average of one hundred billion neurons with an average 7,000 synapses each. For adults this is between 100 to 500 *trillion* synapses.[18] There is exactly one other creature for which the level of specificity sought for the human brain has been worked out: *Caenorhabditis elegans*. *C. Elegans* is a 1mm long roundworm with exactly 302 neurons,[19] making it one of the simplest organisms with a nervous system. A complete structural connection map has been worked out for the *C.Elegans*. This was an enormous undertaking comprising years of collaborative manual eye-under-the-microscope efforts to map every single connection from each of those 302 neurons to each other. As nervous system increase in complexity, the efforts required to map these systems multiply exponentially quickly going beyond the reach of manual labor limits. Given that the human brain's neuronal complexity is several orders more complex than that of the *C. Elegans*, *and* that many of the techniques that are available to researchers of small organisms are unethical in humans, new approaches and techniques will benefit the effort.

Broadly, the Human Connectome Project can be seen as an attempt to find an explanatory rubric by which to see the human brain to interpret and predict its function and structure. The NIH-[20]funded Human Connectome Project's goal, as complex and multifaceted as it is, involves the modeling of various types of data utilizing various imaging and cellular-DNS staining techniques, and amalgamating a variety of data in an attempt to provide future researchers a broad overview of brain anatomy of the kind and detail that we lack today. In addition to structural modeling, the Human Connectome Project also incorporates functional imaging techniques and compares these graph-network models with network results found in the structural models.

I like to think that one of the central intuitions driving this effort is that structure has an important relationship to function, although the exact relationship is, as yet, unknown. Given that many of the methods that are used in smaller organisms are unavailable for study in living human brains, it is necessary to model the brain's neuroanatomy from indirect measures of brain connectivity. While indirect measures, still, the early results from these techniques suggest that much of structural connectivity attributes are mirrored in functional connectivity measurements and vise versa. One of the interesting findings of the human connectome, thus far, has been the ways in which structural network measures overlap with functional network measures (see Network Motifs, Self-Organization Principles, Graph Theory for discussion).

There are important respects in which models play a key role in linking levels of structure and knowledge. Models, by their nature, allow us to link meta-level phenomenon/structure with the lower-level, micro-level activities and units that comprise the larger phenomenon. This is because models are capable of representing at two levels: the level of detail that results from the model simulations and individual data points *and* the level of the model which includes an organizational aspect by way of principles, abstractions, equations/functions depicting an exploratory or proposed mechanism for the behavior of the system. System dynamics will make assumptions in the form of theory. In using graph network theory (discussed in Network Motifs, Self-Organization Principles, Graph Theory) to describe brain networks, the working assumption is that one of the main mechanisms of the mind/brain functioning is that based on network connective properties. This is a fairly sound assumption given what we know about the communication of neuronal cells via their synapses. The details of the submechanisms and supportive mechanisms (such as synchrony, neurotransmitter activity, etc) are unknown, however, given what we do know, it is a worthwhile bet to suppose that much of brain functionality is underpinned by network connections and network connective properties.

Some of the virtues of a model lie, in part, on its partial independence from the data. In the construction of a model, combination of information from different sources and abstractions are necessary since full dependence on either data or on theory is unhelpful. To be useful, models must be hybrid kinds of world data and theoretical parameters. If a model were completely dependent on theory, it would *be* a theory. If a model was just an amalgamation of data, it wouldn't have any further organizing principle by which to conceptualize the system for us.[21] "It is precisely *because* models are partially independent of both theories and the world that they have this autonomous component and so can be used as instruments of exploration in both domains."[22] (Emphasis mine) It is this partial dependence on data and on theory that creates a credible, organizational principle upon which to test further hypotheses, and an avenue by which to 'observe', by simulation of the model, instances which may not be observable. The model becomes an autonomous instrument of exploration that mimicks the object of study in relevant ways. Being an instrument is a more powerful way to view the world and draw inferences about both our theories and our subject of study, but it is *equally important* not to mistake the model for the world itself. Put differently, we must remember that our tools do not necessarily carve up the world at its joints. The best we can understand models are as mediating instruments between the world for our conceptual understanding.

Using a model as an instrument, it is also important to remember that it will always be relative to the domain or relevant aspect of which we are interested. This does not mean that it cannot inform domains outside that of the model's object. However, it is important to realize that conclusions, predictions, and observations made from a model will be domain-relative. Despite domain-relative predictions and observations, it is important to recognize the transformational aspects of a model insofar as it can link across higher levels (model as a whole) and lower levels (individual data points and observations). This duality of models diversifies the influence that any particular model can have to other domains; herein lies their epistemic power.

A model that straddles the two worlds of theory and observation, and thereby remains partially autonomous from both, enables us to test and learn both our theories and predictions about the world. We use models as instruments to build and correct theory.[23] A model can allow us to see system requirements for certain effects or behavior, points of inflection in systems, critical observations in simulations that are not observable or have not yet been observed, and for investigating any characteristics' relationships to each other that are included in or accounted for in the model in some way. In allowing us to visualize or test events, system behavior, and model parameters (ultimately linked to theory), a model provides us with lots of opportunity for hypothesis testing and hypothesis generation. This hypothesis generation and testing will then lead to further modeling, further theorizing, further conceptualizing, further testing, and further explanations. The epistemic power of models comes through their construction and use.

Chapter Two: Reductive Contact

Skeptics, Dualists, Principled Skeptics

Why must explanations around The Connectome make reductive contact with our folk notions of psychological explanation? Well, in short, because The Connectome seeks to understand how structure belies function, or at least, what the relationship between the two is. The Connectome's findings, thus far, suggest that structure and function are closely allied, and in some cases, synonymous. To posit any kind of dualism about function begins to feel like placing a ghost in the machine or just an outright refusal to believe that our mental lives are conducted in our brains and/or our bodies. Further, there seems to be a kind of insincerity implicit in the kind of skepticism that accepts that vision is supported by structures of the brain, but denies that other sorts of thoughts and feelings are not supported by the brain. What is this kind of dualism owed to? What kind of science wants to place more responsibility for one kind of process at the feet of something "mental" while conceding that other processes, supposedly conducted by the same apparatus, as something "less than mental?" What on earth is this?

Well, it might be important to sketch out the kinds of criticisms I'm facing from the skeptics. As Churchland delineates quite nicely, there are two main kinds of skeptics: Boggled Skeptics and Principled Skeptics. I will be taking you on a short tour of these skepticisms with an emphasis on the criticisms of the principled skeptics.

The Boggled Skeptics are the kinds that believe that there is no hope for scientific reduction of mental phenomena to mechanisms of the brain because it's all too hopelessly complex, impossible, absurd, incoherent, or it is otherwise a fool's errand to suppose such reduction is even possible, or that the human brain is as capable of learning about its own mechanisms as it is capable of using them. I won't waste our time with these and allow the already impressive progress of science be my reference upon which my optimistic opinion in response to this kind of pessimism. This kind of pessimism is neither pragmatic nor necessarily true (pragmatism being a virtue in knowledge acquisition), and it seems foolish to spend time trying to defend against what ultimately amounts to a strong opinion without very much support.

Principled Skeptics divide into two kinds of dualists: substance dualists and property dualists. Substance dualists are those that believe that there is something else to the mind that is not just "brain stuff." There is "brain stuff" and "mind stuff" of some description. Generally, these skeptics will talk about this other substance as mind-substance (whatever that is tends to be notoriously vaguely defined), the soul, other kinds of non-physical instantiations of the mind or minds that exist outside the brain (e.g. "Extended mind"), or something that is just "beyond" that substance merely found in the brain. Basically, for them, looking at the brain for the stuff of at least some mental phenomena is just foolish: for it won't be found there. Substance dualists's basic argument form: reduction is impossible because psychological theory and neuroscientific theory are theories about fundamentally different things: mind substances and material substances. And, as such, one will not yield knowledge of the other and vise versa (by parity of reasoning). Brain doesn't inform mind, and mind doesn't inform brain. The kinds of evidence given for these kinds of assertions tend to be the experience of consciousness, what it is like to have thoughts, the qualia of life, if you will. Other evidence given for the existence of non-physical mind stuff points to the human capacity for language and reasoning, which is thought to be unique (although it may exist on a continuous evolutionary spectrum). The main problem with this theoretical approach (besides its potential for being wrong) is that it is often used to justify not just the search for mental phenomena outside of the brain, but *worse* for *ignoring* the brain and the findings of neuroscience, which to me, seems ludicrous and irresponsible science for the sake of maintaing an intuition about mental experience that may or may not be correct.

For property dualists, the case is somewhat different although much of the evidence cited for their case shares many similarities with the substance dualists, with the caveat that the substance is the same, but its properties are different. This case has more credibility and is the more compelling of the dualists' claims mostly in virtue of the fact that they do not have to contend with the difficulty of the problem of the nature of the the two different substances' interactions with each other which, as will be seen, very quickly becomes intractable. In service of property dualism claims, the evidence presented and appealed to also derives from the qualities of experience (qualia), and human wonder at our own ability to reason and use language. The assertion is that subjective experience is irreducible. This assertion, I claim is ultimately an empirical question, not an a priori matter of definition or categorization.

Property dualists further subdivide into Emergentist-Emphasis-Functionalists and Intentional-Emphasis-Functionalists. Emergentists argue that mental properties and the phenomena of psychology are emergent properties of the brain, and in virtue of being emergent, irreducible to properties of the brain. This is a subject of some confusion as the term can be equivocated and used differently throughout the literature. As Churchland elucidates, "Emergent property" is also used in the neuroscientific literature with a quite different sense roughly equivalent to "network property."…Although this is a useful sense of "emergence" (which Dennett calls "innocent emergence"), it is clearly not the sense intended by property dualists in their arguments against reductionism."[24] However, when antireductionists use emergent properties as a reason why neuroscientific theories cannot make contact with psychological ones, they are not simply saying that these properties are simply network properties. If they did, this would be making reductive contact with the neuroscientific aspects of the phenomenon and the goal of reduction would be achieved. The Emergenists in the philosophical literature take it a step further. They argue not only that we can not now identify brain states that correspond to our experiences, but that they *cannot in principle*. This seems needlessly overconfident about what brain states are (which is an empirical question), what they do, and whether or not our experiences correspond to states of the brain or other aspects of the brain, as yet undiscovered. And they do not assert this simply because neuroscience has, as yet, been unsuccessful in characterizing experience in terms of brain states, but they use the short history of neuroscience as evidence that this will never be possible. And it is from this impossibility, in principle, that Emergentists posit the existence of a property dualism in order to do the explanatory work of why neuroscience has not yet made reductive contact with psychology and/or experience.

Another argument often used in support of anti-reduction starts from the nature of subjective experience itself (qualia). The argument is that knowing does not cause or bring about the sensation itself, and thus the experience is irreducible to knowledge of the experience. (Jackson's Mary Thought Experiment) Whether or not this is ultimately true is itself an empirical question. However, there is nothing about the reductive program of explanation that requires that knowledge of a phenomenon cause the phenomenon itself. Causing a phenomenon does not identify it with the phenomenon and no one argues that it should. "Nobody said light did not exist after the reduction of optics to electromagnetic theory; rather, they said light *is* electromagnetic radiation. And nobody said the laws of optics were useless or in disgrace. Notice that on this scenario, it would make perfectly good sense to talk about mental states causing brain states, since mental states turn out to *be* states of the brain (cf. Sperry 1980, Eccles 1977). Nor is a special notion of causation needed, as Sperry (1980) suggests."[25]

Another kind of argument that tends to emerge (pun-intended) from the Emergentist-Functionalist-Influenced camp of thought are those arguments originating from multiple realizability. Notice that the arguments from multiple realizability/ instantiability are just a special case of boggled skepticism and impossibility arguments that we have seen earlier. Multiple realizability arguments take their starting point from an analogy (generally) to artificial intelligence systems, which tend to be a faulty analogy for a variety of reasons of which I will not go into here. It is well known that an artificial intelligence system, and indeed, humans, may be performing the same function whilst their supporting mechanisms may be quite different. Different individuals may be seeking help (in the classic burning building thought experiment) in a variety of ways based on a variety of beliefs about help being needed, but they are ultimately all seeking help. However, the very fact that variance occurs in systems, populations, or, indeed, brains is simply not enough to warrant a claim against the possibility of reduction. The simple existence of multiple solutions to a problem does not necessarily mean that there aren't ways in which it *is being* done, or that those multiple possibilities defy reductive explanation. It would indeed be a prize to behold a science that could not only make reductive contact with phenomenon that we perceive at higher levels of abstraction but *also* that could account for the variety of realizations these phenomena could take. The argument takes for granted that having multiple realizations is inconsistent with reductive explanation. There may be more than one way to skin a cat, but the way this particular cat is skinned, at this time, under these conditions, within narrow-enough defined investigative constraints is *not* a matter of multiple realities. And if there is indeed variation, we are in a better epistemic position to explore the other variations once one version is understood.

Multiple realizability also assumes that in order for reduction to make any sense, there must be a *unique* solution that matches reductive knowledge to higher level understanding of phenomena. This view errs with three strong assumptions of *what a level is*, *that we know* *what parts of biology matter for function,* and *how they interrelate.* This assumption that intertheoretic reduction is a simple one-to-one match up game gives the reductionist program too little credit while assuming that Functionalism has a monopoly on complex explanation via its emphasis on function. Further, this assumption assumes that our higher level understanding of phenomena is perfect as is, and will never be subject to revision. All of these assumptions are potentially false, and worse, insidious for science. Doing science under a framework that assumes what kind of biology matters, what a level is and how levels interrelate, rather than viewing these assumptions as revisable hypotheses risks misleading investigation in the long-term, if not indefinitely and encouraging unfruitful, dead-end types of investigation.

And the final kind of principled skeptic to address are the Functionalists that take the *intentionality* of mental states to be their primary source of evidence for why reductive contact between observed higher level phenomena and lower-level investigation will fail. Intentionality Functionalists take a property dualism as their starting point: mental states are intentional and have content and, for this reason, they are irreducible to mechanism. The content cannot be done justice by the reductive explanation. On this view, contents, representation, and intentionality are objects of explanation which require a different kind of explanation to what reduction seeks. Because this is a functionalist view and the emphasis is on causal relations between intentional states, and these states are irreducible to non-intentional mechanisms, no reduction is possible and investigation can only take place at the level of intention, or at the levels on which speaking about intentionality remains a coherent concept. This view assumes that intentional states must always be described and explained by intentionality, and that there is only one way to access intentionality through representational relationships. Thus, denying the notion that perhaps there is a common linkage between reductive levels of explanation and representational levels of explanation. This is, itself, a controversial view, to be sure, but it seems rather less improbable than minds being composed of sentential-like, representative intentional states all the way down to the lowest level of description. I take Patricia Churchland's view here: "Three brief points on this discussion of the philosophy of language. 1) It is rather far-fetched to suppose that intuitions in the philosophy of language can be a reliable guide to what science can and cannot discover about the nature of the universe. 2) Meanings change as science makes discoveries about what some macro phenomenon is in terms of its composition and the dynamics of the underlying structure. 3) Scientists are unlikely to halt their research when informed that their hypotheses and theories "sound funny" relative to current usage. More likely, they will say this "the theories might sound funny to you, but let me teach the background science that makes us think that the theory is true. Then it will sound less funny."[26]

There seems to be a deep stalemate brewing around the rightful object of study for the cognitive sciences, which I suspect stems from a deep-seated fondness for our own experience of life. Both sides seem to suffer from a deplorable lack of imagination- in what physical structures can realize about experience, and in what whether experience might be realized in something other than a physical structure. Why this argument about structure? Why are we divided on this issue? What is so antagonistic about physicalism? Why is it anathema to so many ears? It is our affective fondness for something "beyond" and something exalted about the human condition? I maintain that this fondness and sense of wonderment for human experience need not be lost in the search. We can maintain (even expand) our childhood wonder and love of human experience even while conducting physicalist, scientific research as to the mechanisms and structures that underpin it, or are, in fact, the realizers of our beloved human experience. It is in fact, unsurprising that we should love our own experience so much: for we know that as a matter of evolution, human individuals that do not experience their experience as pleasurable develop maladaptive depressive coping strategies and, in some cases, display a dangerous tendency towards self-annihilation. Even such knowledge has become an empirical matter whilst its experience remains deeply personal, as it should be. Physicalism is not a campaign to suck the joy out of life; but to ensure that our scientific progress tracks the rightful objects of knowledge.

Functionalists, Functionalism, and function

Part of the assumptions that I make in this essay when talking about brain events that are related to mind events, I take reason of why the mind is probably not (only) a functional kind (see Reductive Contact section). For Functionalist mind "different levels of explanation are linked by the relation of realization and 2 different levels of explanation operate at different levels of functional decomposition."[27] However, I believe that what we call a 'level' from this point of abstraction is actually a deeply problematic issue and remains an artifact of abstraction, when in reality the pleiotropy of genes, synapses and networks on mind phenomena makes demarcating 'levels' much less clear. This is partly why I believe that modeling network effects is a promising avenue within which to explore mental phenomena links with lower 'level' phenomena. So I believe modeling can bridge these explanatory gaps and find reductive contact with 'lower level' action. Schemas and dynamic model exploration will link the abstractions of global (& mental) system behavior to brain behavior data points. I therefore propose that a minimally functionalist notion, coupled with a strong physicalism is a necessary view in our mind/brain explorations.

SO, I would like to defend the importance of the place of function in any theory of neuroscience and cognitive sciences without espousing the kind of Functionalism that entails with it particular kinds of ontology about mental states and what kinds of explanations will ultimately suit mental phenomena and carry causal explanatory weight. To start with, I'd like to distinguish between what being a Functionalist (with a capital F) entails and what the role of distinguishing and highlighting functional types with regard to mental phenomena is. Functionalists carry ontological commitments to what kinds of explanation will ultimately satisfy our interest in psychological explanation of mental phenomena. The Functionalist typically envisions that a mental kind (this is the ontological commitment) is going to be a phenomena of a functional kind such that it is multiply realizable by the brain substrate that supports it, which thereby renders the project of reduction and reductive explanation impossible. Function distinctions, on the other hand, are quite necessary to the progress of neuroscience and the project of discerning the kinds of processes that the mind/brain is capable of conducting. Hereforth, I want to make clear that I am taking issue with a particular flavor of Functionalism that takes the very ontology of function to be of an irreducible quality such that levels of explanation between neurobiological substrates and causal, mental phenomena will never be realized (despite the physicalist view of the Functionalist). Function, as I will use it will be an entirely unspooky phenomenon and will be a kind of explanation among the many types when it comes to the object of study: mind/brain.

Primarily, I will be combating kinds of Functionalism that posit functional kinds as a rarefied type that cannot be explained by any reductive material kind. This is not to defeat that notion that it may, in theory, be possible for a function to be realized in the brain in a number of different ways, but only to deny that the simple possibility of multiple realizability of a function won't preclude a consistent reductive material explanation at some appropriate level of explanation. Further, I maintain that it is perilous for research to methodologically assume a priori what structure or what function will be doing the explanatory work for the macrophenomena for which we seek explanation. The main point here is that it is important, while doing exploratory research, to maintain a level of agnosticism as to whether a functional kind, multiply realizable, or a structural kind, multiply flexible and/or optionally operationalized will explain any particular meta-categorized mental state, as we currently describe them.

A further problem, which I will explore a little bit further on (see Language Matters! Reduction & Categories of Folk Psychology), is that functional kinds, as we currently delineate them, are very probably incomplete, inconsistent or incoherent. This is a view that functionalists and neurocomputationalists alike hold. It is likely that what we envision as 'memory' or as 'consciousness' very likely has various aspects, components, or clusters of functionality, if you will. As we find better ways of describing what the brain actually does, we may find better ways of conceptualizing the macro level phenomena that we seek to understand, and in the process, we may gain other ways of describing the macro level phenomena that better fit their function and structural implications. J.Z. Young puts this point nicely: "[f]urther information should allow us to replace the single concept of mind and mental activity by others more fully descriptive of the modes of action of brain processes."[28] (J.Z. Young, 1978)

In order to make the distinction I am drawing clear, I'd like to bring out a few features of the kind of Functionalism I'd like to dispel in our explanatory framework in the use of The Connectome findings. "In general, functional kinds are specified by reference to their roles or relational profiles, not by reference to their material structure"[29] This is a useful notion to keep in mind while exploring what both the brain and the mind do. In denying views of functionalism that rule out reductive possibilities, I maintain that a view towards understanding function, both of the brain and of the mind, remains critical and important in the study of the mind/brain. However, the kinds of views I firmly believe are counter-productive are those that 1) maintain that our folk psychology categories of mental states, as rooted in our conventional language, are fundamentally correct (They are certainly dearly and affectionately held, to be sure, but "correct" is another matter.), 2) that these folk psychological categories delimit intentional states and logical processes to be explored, uncovered, and whose mechanisms will be discovered by science and 3) that these folk psychological categories will not reduce to neurobiological explanation. The upshot of these kinds of views is that neuroscience is unimportant to and can be conducted more or less independently of psychological study. "The implication for research in the relevant domain of scientific psychology is that neurobiology is largely irrelevant[!] to discovering an adequate theory of information processing at the psychological level."[30] I believe the conclusion of this argument is in various unstated ways *actually motivating* some of the premises of the argument. Namely, I believe that, at least in some cases, a lack of understanding and a fear of the complexity of neuroscientific mechanisms, categories, functionalities motivates a desire to keep psychological functional investigation somewhat distinct and separate from neuroscientific "mechanistic" understanding (more later on why "mechanistic" may be an unsuitable descriptor of neuroscientific knowledge). To be sure, this is not a view all psychologists hold, and many do not. However, it seems to be a view that consistently wins sympathy among philosophers and some psychologists and this seems to me a pernicious view that seems to perpetuate and encourage dualist views of what the mind/brain is *a priori* to research. The strong assumption here is *that we know at what level biology matters*.[31] "We simply do not know at what level of organization/implementation one can assume that the physical implementation can vary but the capacities remain the same."[32] Further, assuming that we know at what levels biology matters leads to important assumptions about the kinds of research that will be productive and will shape the efforts of our scientific enquiries. I believe this is a very high stake which we should be more cautious in approaching when arguing for a view of the kinds of science that will and won't be productive. It is simply *not* the case that we know at what level biology matters for the kinds of explanations we seek, and knowing what a level is, and what matters in biological explanation is a subject that I will go into later in my discussions around levels of explanation in biology and The Connectome.

Thus, immediately, there are three points to address here in regards to this argument:

1) that folk psychology categories of our conventional language are fundamentally correct

2) that these categories are intentional and logical states, to be explained by neuroscience mechanisms

3) that these are non-reductive to neurobiological states

In the following section, I'd like to pursue these three claims of the of the non-reductive physicalist, in detail, in an effort to illustrate the ways in which neurobiology *is* relevant to the investigations of psychology and the categories that folk psychology cares about and seeks to explain.

Before moving on to these three claims of the antireductionist physicalist, I'd like to briefly express a positive view of the kind of functionalism researchers ought to hold. The study of function, after all, is useful and necessary for the progress of the cognitive neurosciences. "The thesis that mental states are identified in terms of their abstract causal roles in the wider information-processing system is the core conception that makes functionalism functionalism, and it is entirely neutral on the question of reducibility. Functionalists can be true blue functionalists without naysaying reduction."[33] Function should be recognized as an exploratory tool in cognitive sciences. Function is, afterall, a category that we impose upon the workings of the brain/mind from our point of view. But recognizing that we are categorizing behavior by function relevant to human experience doesn't automatically mean that the function doesn't exist or that it isn't a viable way of categorizing experience and/or brain function. We simply do not know from this standpoint, yet, whether or not the functional categories that we currently have are the best ways of referring to the functionalities that the brain/mind *does* have (vs. the ones that we hypothesize they have).

Language Matters! Reduction & the Categories of Folk Psychology

It's all very good to speak folk psychology in therapy, among friends, in the pub, at home with spouses, children, and other relatives.

However, I suspect that the reason why people are so defensive of folk psychology is because they have an emotional/cultural attachment to it, as is natural for the everyday language we all employ. However, just because we are currently attached to the words that we use to describe our inner states does not mean that we have always used such words, that they are or ever have been accurate ways of describing our own states, or that we are entirely un-influenced by the discoveries and trends in the cognitive sciences. After all, why would folk psychology even *be* influenced by such terminology as "Oedipus Complex" and "Attachment Styles" if it weren't for Freudian psychology and modern affective psychology? To deny that folk psychological concepts evolve or should evolve is just to be historically naive. Folk psychological concepts come in and out of common parlance and in and out of disuse according to current knowledge and discoveries. To suggest that our ways of relating to each other through folk psychological terms may evolve is simply to acknowledge what already *does* and *will* happen. This does not mean that we have to start speaking a strange language of psychology that none of us understand amongst ourselves right now: merely that as our theories of psychology evolve, so, too, will our language of relating to the things that are psychologically relevant and important to us.

Why are we so enthralled with keeping a language that doesn't work for us? It must be because of the vestiges of the emotive commitment they carry: we want to know that love is love, that fighting is fighting, that pain is pain and not something else. But you know what? They will still mean what we agree that they mean: culturally *and* referentially. They will continue to have their social uses and will continue to mean what we need them to mean because they are relevant affective parts of our environment. Much of affective appraisal theory takes this kind of human social necessity as a starting point. Whether we call pain p-a-i-n or twenty different subtypes depicting different aspects of it, our cultures, groups, families and societies will still pick out the relevant features for our well-being and use the relevant language because that is what we have evolved to do: whether we call it *dolor* or pain or if it remains unnamed. *And* pain will even have the same functional features and same relevancy so long as humans are built to respond to it.

Other examples of our evolving psychology language include attachment theory, reverse psychology, psychology of motivation (spanking vs rewards), and conditions such as depression, bipolar, schizophrenia, Down's, Parkinson's, Alzheimer's. Understanding of all of these things has evolved, for example we not longer believe that people are possessed by evil spirits when they are found to have some kind of mental deficit. As a result of evolution of conceptions of psychology and what the mind and brain do, we very often find that new terminology enters common parlance. Even now, we find instances in which our current language somehow fail to characterize adequately or fully for the kinds of capacities or deficits that the mind/brain has. Memory, for example, is not a unitary concept for cognitive psychology. When it is broken down theoretically, there still seem to be gaps where behavior defies theoretical characterization. Aspects such as visual, auditory, and working memory seem to share some capacities in common while some aspects are dissociable. What, exactly those shared and unshared capacities are, and how they can be affected by age, sex, language, culture (just to name a few factors) is still the subject of inquiry and definition. As our understanding grows and evolves, we may even find that some of the capacities that we had ascribed as mental are somehow hybrids of mental, bodily, and cultural capacities.

I am defending Patricia Churchland's eliminative materialism. "By "eliminative materialism" I mean the view that holds 1. That folk psychology is a theory; 2. That it is a theory whose inadequacies entail that it must eventually be substantially revised or replaced outright (hence "eliminative"); and 3. That what will ultimately replace folk psychology will be the conceptual framework of a matured neuroscience (hence "materialsm"). (For philosophers who defend this view, see Feyerabend 1963b, 1963c, Rorty 1970, Paul M. Churchland 1981, Stich 1983. For neuroscientists whose views are very close to this, see Young 1978 and Crick 1979.)"[34]

So why hold on to old categories from our social settings in cognitive science if they are clumsy for research? There is no need, and further, it causes more harm than good. Folks may not like to hear or not always be interested in understanding the advances of psychological science and the niceties and distinctions of behavioral and molecular biological science just as folks may not be interested in the minutiae of the mechanism of high blood pressure, but will still be interested in knowing what kinds of foods may ameliorate the condition (because it has relevance to their well-being). For all currently foreseeable pragmatic and intensive purposes, this suits us just fine. However, the debate needn't be carried into science where it could do more harm than good. After all, if scientists were concerned with sorting out the movement of the spheres instead of gravitational pull, we'd have much less progress in physics than we enjoy today. And our society would be worse off for it. "Even if folk psychology is in some degree 'built in,' as perhaps folk physics may be, such innateness does not guarantee its truth, its adequacy, or its immunity from revision."[35] Ultimately, in defending a language of eliminative materialism, I am advocating a kind of theoretical openness. In maintaining an open attitude to the kinds of characteristics of the mental and physical, we allow greater space for interpreting future discoveries of the mind/ brain. In allowing this theoretical space, we'll have the conceptual space to consider without ontological conceptions about what exactly it is we are looking at.

Even if you don't buy this argument about Folk Psychology categories, the ontological commitments that folk psychology carries with it hinders progress in science because it mistakes the name of a phenomenon for what the phenomenon *is.* The assumption that certain levels of explanation are functional levels, while others are the mechanistic underpinnings grossly assumes at what levels functions and mechanisms exist and interact. Even if one lets go of any particular kind of nomenclature for constellations of behaviors as unimportant, the thrust of the issue that concerns me about Functionalists and needlessly hanging on to the categories of Folk Psychology is the problematic rich ontologies that these kinds of views presuppose. Functionalists tend to 1) have a notion that there is a one-to-one match up of intertheoretic reduction, *and* 2) tend to have an erroneous conception of what a level is, and 3) how they interrelate.[36] Often, the assumption that intertheoretic levels of explanation must have a one-to-one match up is piled on top of Functionalist views that 'mind stuff' will necessarily have a different type of explanation than 'brain stuff.' Functionalists will categorize 'mind stuff' as functionally multiply realizable and only suitably described by a functional-level explanation, while brain stuff will only suitably be described at the 'brain-level' by brain parts, with the implicit assumption that those brain parts are not also mind parts. This is to assume what parts, level, and kinds of biology matter, what they do, what functional significance they carry. Even if mental states are functional states (which they may very well be!) this does not mean that they can not make reductive contact with what occurs in the brain or that they are not, in fact, *also* brain states.

However, "it is evident that there are *many* levels of organization between the topmost level and the level of intracellular dynamics. (See also Lycan 1981a.) And even if there were just three [levels], neurobiological theory challenges that way of specifying their organizational description. How many levels there are, and how they should be described, is not something to be decided in advance of empirical theory. Pretheoretically, we have only rough and ready-and eminently revisable--hunches about what constitutes a level of organization."[37] I propose that some these 'levels' and the ways that they interrelate are objects of knowledge that can be at least partially identified by the Connectome Project, particularly with regards to networks of the brain. For the meantime what we have, given the biology and psychology vocabulary that we have, are preliminary ways of demarcating levels, networks, and assemblies. Fodorian modularity was one such way of characterizing levels. Some ways of characterizing levels will come as a result of our methodologies and we should be careful to account for those effects. Because these are just preliminary ways of characterizing networks, there isn't any need to force a pre-conceptual folk psychology theoretical fit to our research findings. Further, it is eminently clear that at every 'level' of research there are functional questions, questions about the nature of the capacities, questions about mechanism and implementation, questions about processes. "The point is, even at the level of *cellular* research, one can view the cell *as being the functional* unit with a certain input-output profile, as having specifiable dynamics, *and as having a structural implementation* in certain proteins and other subcellular structures...Relative to a lower research level a neuroscientist's research can be considered functional, and relative to a higher level it can be considered structural...The structure-function distinction, though not without utility, is a *relative*, not an absolute, distinction, and even then it is insufficiently precise to support any sweeping research idealogy…There is a further assumption, usually unstated, that lends credence to the ideology of autonomy [of neuroscience from other cognitive science disciplines] and should be debunked. This assumption is that neuroscience, because it tries to understand the physical device--the brain itself--will not produce *theories* of functional organization…[However,] in doing so, there are up to their ears in theorizing, and even more shocking, in theorizing about representations and computations."[38]

An objection that gets brought up over and over again against the eliminative materialist is that it is impossible to talk about the states of the mind/brain that he wants to talk about without reference to current folk psychological language and/or theories, which then requires the eliminative materialist to adopt the categories of the folk psychologist. My response (and Patricia Churchland's) to this is simply to acknowledge that we have the language that we have now and simply because I need to use it doesn't mean that I believe it's the best way to describe or characterize the phenomena which we investigate. It simply means we don't have anything else at the moment. “[T]he phenomena that need explaining are specified in the vocabulary of the available theory (for example, the turning of the crystal spheres, the possession by demons, the transfer of caloric, the nature of consciousness). To tender sweeping criticisms of the entire old theory while still within its framework will therefore typically sound odd.”[39] This is to be expected in the process of revision and when one theory is moving towards a new view with accompanying vocabulary. Needing to use outdated vocabulary for the time being isn’t a criticism of the project of finding better or more accurate characterizations.

Reductive Contact

Much of the controversy around the nature of reduction surrounds the very real and somewhat visceral intuition that the mind is not the simple sum of its parts. There is a concept, that I am going to borrow from heavily from in the foregoing discussion of ‘levels’, in genetics called pleiotropy. Genetic Pleiotropy is when a single gene influences multiple phenotypic traits. This effect has been found to be ubiquitous for many many phenotypic traits. Often, altering one gene will have an effect on some or all of the phenotypic traits simultaneously and often in ways that we are not yet able to reliably predict. For genes, if you consider selection on one phenotypic trait, it’s entirely possible (probable, in fact) that the gene, genes, allele, and/or alleles (which is a version of a gene) that the trait is supported by will have effects on other phenotypic traits. These effects may not always be desirable and may sometimes have competing results of which some will be beneficial while others detrimental to the organism while have neutral effects for the organism in other cases. Thus, selection on a surface feature (phenotypic) will have complex effects on the system and vice versa. Often a phenotypic trait will also be supported by a large number of genes and/or alleles. So, selection of one gene or one allele will not necessarily impact one phenotypic trait in a straightforward or predictable way. Selecting for a part at the level of the system and vice versa will not often have one-to-one effects. It is also not always clear to as at the current moment at what ‘level’ a gene is supporting the phenotype. A gene may simultaneously affect sodium channel functioning in particular parts of the body while affecting heart rate, which can then affect many other functions of the entire body. Knock-on effects of genes are in addition to those that are multiply supported by a plethora of genetic factors and effects. Complexity is inherent.

Complexity is a fact of the human body and living organisms that is used both in support and in attack of Functionalism, Eliminative Realism, Dualism(s), Representationalists, and pretty much everyone. It is an issue everyone uses for their purpose. Complexity may not necessarily adjudicate in favor of eliminative materialism, but it certainly doesn't speak against it. The fact that a function may be multiply realizable or multiply supported by various physical structures of the brain and body does not mean that it will be impossible for any particular mental function to be described, characterized or otherwise make reductive contact with the operations and physicality of the human brain. What often appears to multiple realizability arguments as an insurmountable difficulty about reduction is simply that our higher 'levels' of explanation are not going to have tidy correlates at the lower 'levels' of brain function. Just as a gene may code for a whole family of phenotypic traits, and one phenotypic trait, as described by us now, may be supported by whole groups of genes, various mental phenomena may be supported by various conglomerations of brain functions, various groupings of networks and processes that we do not yet understand or have entirely clear ideas about what it is they do, exactly. So, from the meta-level of description it may very well seem hopelessly simplistic to describe such complex mental phenomena as particular classes of belief in terms of neuronal outputs, given the variation among individuals and lack of clarity that we have about functions at all levels. It is for this reason that I favor an openness to new vocabulary of mental phenomenological states as we understand more about what the underlying processes are. We should be favoring a co-evolutionary approach between classes of disciplines that focus on 'mechanisms' and 'functionalities'. The more we discover about both function(s) and mechanism(s), the more apparent it becomes that these two things can not always be segregated either experimentally, functionally or physically. Take even the case of those studies of brain resting state imaging (many of which are used to support some conclusions of the Connectome Project)[40] where it is debated what functionalities can reasonably be inferred[41] about the 'resting state.' Since human brain science isn't conducive to static observation, taking account of all of those ways in which the system remains dynamically engaged at any given point becomes very important and acknowledging the inseparability (to some basic extent) of mechanism and function is an important part of the puzzle. The fact is that discoveries at one 'level' will have consequences for other 'levels' and they will each inform one another.

Part of the worry that I have with the concepts of folk psychology and even with some high-level functional concepts is their imprecision. Although many functionalists (and this isn't everyone) concede that many functions will not be neatly supported wholly and conclusively by one or two mechanisms in a clear and direct manner, there isn't a whole lot of energy that goes into discussing what those components might be or how neuroscience might contribute to their functional categories or to the reconceptualization of the nature and organization those functional categories, even though it is often admitted that those same functional categories and conceptions are less than ideal.[42] Take for example the continual refinement of what functions memory uses, what various tasks the category of learning includes, and what the variety of phenomenon is generally meant to be encompassed by consciousness. Without more refined conceptualization and categorizations, hypotheses remain general and clumsy. It seems to me that if a category is misconceived in part because of the conceptions that the current language carries, there ought to be an opportunity for a new category ,with attendant refined conceptualization, to take hold. Rather than just fleeing immediately to the assumption that because something can not be simply and directly explained it must be epiphenomenal. This is not to say that it may not, indeed, turn out to *be* epiphenomenal. But the point is to withhold judgment on that fact until more is known about the complexity of the mechanisms, how they interrelate and the possibility that there is another kind of causal physicalist explanation that is possible to discover ahead of assuming it is epiphenomenal.

Patricia Churchland uses a very nice analogy taken from Dewan 1976 with what is known in engineering as a virtual governor. Briefly, a virtual governor is a network of energy generators that each individually produce within 10% of a particular frequency of electrical power due to fluctuations in current generation. However, when you put these generators into a linked network together, their fluctuations are much much less than 10% and pretty near negligible because, statistically, in a network one generator's overproduction will be counterbalanced by another one's dip. Together, they consistently deliver a particular frequency and are said to function as one generator: a virtual generator.[43] The system, as a whole, can be said to deliver current of a particular frequency while the same cannot be said of any of its individual components. However, the effect is clearly attributable to the whole. The question for reduction is this: can one reduce the function of the whole further than at the system level? No, the particular function "cannot be localized more closely than the system as a whole."[44] However, the phenomenon is composed of each of the generators and the phenomenon as a whole could not exist without the network of generators, each individual component and all the relationships and functionalities between them. I am in agreement with Patricia Churchland (and others) here insofar as I believe that many current categories and conceptualizations of functions of the brain will turn out to be kinds of virtual governors.[45] They are meta-level conceptions and observations that we have at this moment to describe phenomena and functions of the brain. However, these are imprecise as to what the functionalities are that support what we observe and it is misconceived to assume that those levels will match up with the meta-levels in any clear and direct way. It's possible, but not looking likely. In reduction, the point is that our theories make *reductive contact* with the physical levels in such a way that the possibility of explaining causal relationships between physical states, mechanisms, and structures is at least possible. There is good reason to believe the the complexity of the mind/brain will lead us in this complex path of reduction because "this tends to be the case the more levels of organization there are in the system and the more complex the route between input and output, and because of these and other complex factors, the characterization of input-output functions and input-output laws will be revised to mesh more closely with lower-level discoveries."[46]

In a co-evolutionary approach, neuroscience and cognitive psychology work hand-in-hand and side-by-side to adapt, refine, and conceptualize 'levels', mechanisms, and functions of the mind/brain. Some examples of research where reductive contact can be made between 'levels' (and richer reductive contact is hoped for and expected) would be studies such as those of"Quinn and his colleagues (Quinn and Greenspan 1984) [who] have found a complementary account to explain why certain mutant populations of Drosophila are learning disabled, and thus a connection has been made between specific genes and the production of an enzyme known to have a causal role in learning…Vertebrate nervous systems are much more complicated than those of invertebrates, but the Aplysia research has provided a framework of hypotheses that structures research on the cellular basis of habituation, sensitization, and classical conditioning in the vertebrate brain."[47]

Further frameworks for hypothesis formation can be expected from anatomical models such as the Connectome Project aims to complete. Anatomical models can provide large and various amounts of qualitative data about connections and the kinds of connections between neurons and brain regions. Just as it is helpful to know in what direction a knee joint bends for inferring the kinds of movements that a particular creature can perform, it is helpful to know the kinds of neurons that are connected to each other, whether they are reciprocal connections, whether or not they are inhibitory or excitatory connections, what kinds of receptor sites exist, etc. This is the kind of knowledge that assists researchers in building good artificial intelligence models to test the kinds of interactions that such networks have and are capable of conducting. Network behavior results from such simulation experiments can then be compared against the behavior of such networks found in humans and other vertebrates to test to see if the kind of connection and behavior of the connections hypothesized by the model imitates behavior found in nature in revealing ways. For creating useful modeling tools, anatomical studies of naturally occurring intelligent systems are vital for testing parameters and making inferences about connectivity and mechanisms. A great example of a study in which anatomical studies and anatomical information was deeply influential in hypothesizing was: “In one of the latter experimental attempts Berger, Thompson, and their associates found that after training there was a sort of cellular representation of the behavioral response in the hippocampus, and this “representation” developed during the training phase of classical conditioning (Berger and Thompson 1978, Berger, Latham, and Thompson 1980). In the performance of the learned response, it occurred prior to the onset of the behavioral response. Interestingly, however, lesioning of the hippocampus does not prevent classical conditioning, thought lesioning of the interpositus nuclei of the cerebellum does. Anatomical studies reporting pathways and connections have formed the basis for the hypotheses to explain the effects, and these hypotheses were important to those studying humans and those studying animal models.”[48] This is exactly the sort of study that the Connectome Project will inform and be useful towards. Uniting lower 'levels' with higher 'levels' requires intermediary knowledge of structures and organizations that link the levels. Mapping neural connections may be one such of those organizational maps.

Chapter Three: The Road Ahead

At What Level(s) does Physiology Matter?

"What the mind-brain is doing—even as described at the level of input-output functions of the system—is not an observational matter, to be read off simply by looking at the behaving organism. Rather, it is a deeply theoretical matter...The heart of the matter is that if there is theoretical give and take, then the two sciences will knit themselves into one another."[49]

One-sided views of mental phenomena that assume certain kinds of biology matter while others are relegated to the pile of "mechanistic implementation detail." Part of what I find irritating about these views is that the division between biology 'that matters' for explanation and biology that 'doesn't matter' is fundamentally an artifact of the ways we conduct scientific inquiry, not necessarily a reflection of a division in the natural world. As David Marr, Daniel Dennett, Bermúdez, and others have laid clear, there are various levels of description and scientific inquiry that yield different kinds, types, and levels of explanation. I think that we have to be careful between confusing intuitions that we have about levels of explanation at the personal level with scientific inquiry that will be conducted at the subpersonal level.

Briefly and generally, the personal level of description encompasses and distinguishes itself from subpersonal levels of description by three hallmarks: accessibility to consciousness, cognitive penetrability, and inferential integration. Accessibility to consciousness at the personal level of description would be that group of phenomena that an individual would have access to consciously. Cognitive penetrability would be that group of states that would be impacted by an individual's attitudes, beliefs, emotions and other propositional states. These states would be influenced by changes in these cognitively penetrable attitudes and beliefs. For example, a personal level phenomena that is cognitive penetrable would be changing one's seat on the bus if one believes that the person sitting next to them is dangerous. This would be a cognitively penetrable personal state. Inferential integration refers to a modification of the principle of cognitive penetrability as an identifier of the personal level. Inferential integration refers to the direction of influence that a propositional attitude (such as belief that a person sitting next to you on the bus is dangerous). The idea behind inferential integration would be a further requirement that a cognitively penetrable state would be further identified by the way in which inference from an attitude would be sensitive to a personal state or a personal state would be inferentially sensitive to a propositional attitude.[50] However, as Bermúdez points out, "It looks, therefore, as if none of these proposed criteria can on its own demarcate the realm of the personal level— and nor, of course, should this be very surprising. Hardly any concepts of theoretical interest can be captured within the scope of a neat set of necessary and sufficient criteria. It is true, nonetheless, that the disjunction of the three proposed criteria is a useful tool for picking out personal-level states— we can be pretty confident that any personal-level state will be either accessible to consciousness, or cognitively penetrable or inferentially integrated. But, as one would expect from a disjunction, it tells us little about the real nature of personal-level states. For that we would, I think, be better advised to look at the explanatory role that such states are called upon to play."[51]

Given that there is an important link between how we conceive the ontology of the mind and how we explain it, it's important to see how our explanations are influencing our conception of mind/brain ontology and how mind/brain ontology are influencing our conceptions of explanation. The case I want to try is that of how the antireductionists' claims may be based in a mind/brain ontology that influences what kinds of explanation are appropriate. Given that mind/brain ontology is the thing about which we are curious, it seems to me hardly an issue that can be decided before seeking explanation. Further, since we seek explanation, we must be highly conscious of the ways in which our methodologies of explanation impact our ontological views of the mind/brain. The contention I make is that there are particular kinds of scientific bigotry which bias researchers against seeking what may very well be a reality of the subject of research simply because their methodologies support and privilege certain kinds of explanations and ontologies over others. What I am ultimately advocating is a kind of openness.

For example: David Marr distinguishes between bottom-up and top-down levels of analysis. Dennett also distinguishes these levels in a way which I feel is more faithful to their sociopolitical science properties: intentional stance, design stance, and the physical stance. When one studies the mind bottom-up, according to Marr, one is analyzing at the level of neurons, molecules, pathways, and action potentials. When analyzing from the top-down, one is taking function, thought, and corresponding mechanisms as the starting point of analysis. The insight Bermúdez lends to these analyses is that they lend themselves to different disciplines of study and analysis. This is no small point. In advocating one view over another, one must try to remember where your analysis is coming from and what methods and inferences and background knowledge motivates it. The dream of the materialist is to unite these levels of explanation. Or, better put, as Churchland says, to find reductive contact between levels. Meaning that at the appropriate level of description, the physical material explanation will describe the meta-level phenomena we observe at the level of the person. The resistance that comes from the antireductionist camp seems to be premature and needless amount of skepticism about whether or not reductive contact can be made between levels. This is often referred to as the interface problem: how do these levels of explanation match up?

Dennett's levels of explanation seem to be better in keeping with the methodological aspects of the types of explanations as well as hinting at the kinds of impacts and inferences these levels of explanation will imply for the consumers of these levels of knowledge. The intentional stance can be roughly said to line with with the functional, task-aspects of explanation. At this level general strategies and system-level considerations are examined. Next, the design stance considers general constraints and principles (in the absence of "laws" in the biological sciences). This level is still relatively flexible in terms of the number of possibilities which could solve the general problem of performing a task, but it is more constrained than the intentional stance level which is the highest and most general problem-definition level. Finally, there is the physical stance which considers and analyzes how a task and solution system set could be physically constructed and implemented/evolved. The top-down analysis approach begins at the functional level and works its way down by chains of inference to the physical implementation level. The bottom-up approach begins at the level of physical implementation, intending to understand it as best it can, while trying to understand how these physical implementations may match up, impact, or otherwise inform higher levels of functional explanation. Bottom-up approaches may start from relatively narrow physical constraints with a fairly limited set of functionalities that are understood, with a seemingly unimaginable number of ways that these constraints could affect functionality at higher levels. However, through analysis, and in conjunction with information about higher level functioning and behavior, these base physical "realizations" of higher level function may be further constrained and elucidate in providing explanation.

The anti-reductive Functionalist (capital F) essentially puts forward his claims for methodological reasons: the Functionalist, focusing at the top levels of analysis carries out a classical top-down approach which provides fewer constraints for realization at the bottom level than an approach which conducts a bottom-up approach to analysis. This is where the argument from multiple realizability builds a lot of momentum. The very notion that a particular function may have various realizations is enough to set any theorist's mind ablaze with possibility. However, the charge the strong Functionalist makes against the material reductionist is that the bottom-up approach isn't going to provide any *real* insight about function. This claim seems to be just wrong and overstated. First, no such opposing view exists. There aren't any reductionists that claim that function is *unimportant* or marginal to their research, and many of them are constantly taking the behavioral functional inputs observed as information to fuel their bottom-up level inferences. Function is vital to the reductionist. One reason I have to oppose this kind of dismissive methodology is that I think it's lazy and overconfident that functionalism has a privileged view into the nature of cognitive systems that *any* bottom-up approach could not hope to have. I believe that the only way to make meaningful discoveries about the mental life of humans and intelligence in biological systems is to conduct analysis from both levels simultaneously and in a co-evolutionary way such that both sides of analysis interact and inform each other. Certainly there are better and lesser scientists, but I do not think that in this instance functionalism should have a privileged route to knowledge over physicalism. These two approaches can coexist, and better yet, interact fruitfully to jointly reinforce our knowledge. And in their cooperation towards a theory of mind, the point is that both approaches have equal stake in the kinds of knowledge that are sought, and that both function and physical implementation will always be jointly important for explanation. I also believe that function and physical systems will be reciprocally exacting and constraining towards each other when taken jointly into account as part of any particular theory of explanation or any one given kind of explanation. But the richness of theoretical explanation depends on the coherence between function and physical implementation and vise versa.

The real question, then, that faces the physicalist is: can we reverse engineer (fruitfully to gain knowledge) the mind/brain from the level of neurons, networks, action potentials, gamma waves, synchronized firing, white matter networks? Can we really learn what function or task the mind/brain was constructed to perform? Can we learn at what level our neurobiology matters for higher level phenomenon explanation? And, if we can backwards engineer what the mind/brain's functionalities are from the level of neurons, what problems does variability present for analysis and abstraction of function from lower level physical systems?

I contend that these hard questions of the possibility of reverse engineering, inferring something about function from structure, and discovering at what level(s) neurobiological realization matters for higher level phenomena explanation are all things that are questions to be empirically tested. These are questions, some of which, the Connectome Project is particularly well positioned to learn about initially and perhaps, in the long run, solve. Whether or not the Connectome Project will solve all of our questions about the nature of intelligence, minds, and brains is unlikely, but that is not the goal. The goal is enriching, in a meaningful way, that will further our knowledge of the mind/brain and lead us to more interesting and more detailed research questions about the mind/brain. The Connectome is an attempt at framing some kinds of explanatory mechanisms by which the mind works. We know that brain networks play a role in brain function but we do yet know what those exact roles are. It is a matter of inquiry whether or not the network effects are the effectors of the mind or just a manifestation of it. But, right now, as a nascent science, neurobiology and neuroanatomy is vitally important in discerning connections, both anatomical and functional, that will inform more targeted inquiry by both functional theorists and physical researchers. Prior to particular knowledge of connectivity and what kinds of processes such connectivity (structural and functional) will influence, it seems premature to adjudicate on what level(s) physiology matters. This is just a matter of exploration at the moment. This is a mark of a nascent science.

Network Motifs, Self-Organization Principles, Graph Theory

"In this heirarchy, no single level is privileged over others. The notion that brain function can be fully reduced to the operation of cells or molecules is as ill-conceived as the complementary view that cognition can be understood without making reference to its biological substrates."[52]

So— , you ask, what can networks and connectomics inform us about the mind/brain, its function and its *nature*? First, I'd like to say that I believe the question about the *nature* of the mind/brain is entirely too vague. Different people mean vastly different things by this question, and it is a question that is itself decomposable into parts, some of which are already disciplines of inquiry. So, I'm going to shelf the issue of so-called "nature" of the mind/brain as a question that is answered in part, by this entire exploration into what the ontology of the mind is, what kinds of explanation we can expect from network theory, and what kinds of questions about the mind these explorations will motivate. What I suggest the connectome is is a new, helpful method for organizing information that we gather about mind/brain structure and functionality in order to organize further hypotheses and hypothesis testing.

So, turning to function and mind, I'd like to outline some interesting preliminary findings about networks which are currently being used to analyze connectivity. The devil is in the detail, after all. Network theory can be used to measure several parameters of connectivity, which I'd like to outline and describe here, for reference. These measures include: node degree, degree distribution and assortativity, clustering coefficient and motifs, path length and efficiency, connection density as a measure of cost, hubs, centrality, robustness, and modularity (not Fodorian Modularity). See inset Box 1.

There exists a phenomenon that has been characterized across other disciplines that use network theory analysis called 'small world networks'. Small world networks[53] are characterized by the efficiency and connectivity of a graph. See Figure 1. (The figures will be helpful throughout this section.) Hubs are highly characteristic of scale-free networks which tend to self organize themselves into small-world network properties that enable efficient communication between neighborhoods. For a good visualization of a scale-free network please refer to Figure 2. Scale-free networks are characterized by power-law degree distributions. Power-law degree distributions are mathematical, statistical ways of characterizing the number of connections that go through any one particular node. Graphically, this can be seen in "hub" small-world kind of organization. Hub characteristics include high clustering and short path length, which are measured against a random network's measurements of clustering and path length. Small world characteristics can be conceptualized as evolution's compromise between efficiency and cost. A highly efficient connective graph can be conceptualized as one in which every node is connected to every other node. However, this needs to be balanced against the reality of limited resources and the cost that it takes to maintain each connection. This can be conceptualized in graph theory as a relative measurement of density. A less dense graph will have lower costs associated with it than a more dense graph. So, small-worlds solve the compromise, by essentially picking a point through which their messages can be routed through to connect to other parts without sacrificing their connectivity and maximizing efficiency by maintaining short path length (despite the many times in which there will be an indirect connection between two individual nodes). So, efficiency and cost are key diagnostic measurements of whether or not a particular network has small world properties, which tend to be characteristic of complex networks.[54] Efficiency and cost are measured by path length and connection density, respectively. Wonderful illustrations of these differences in connectivity efficiency and costs this can be seen at Figure 3, Figure 4, and Figure 1. Some examples of natural world phenomena that exhibit small world properties are: road maps, food chains, electric power grids, airline flight networks, metabolite processing networks, voter networks, telephone call graphs, and social networks.

Small-world properties are endemic to complex networks.[55] They are found at every level of biology from cellular networks to social networks to networks of connected proteins. Similarly,[56] transcriptional networks, in which the nodes are genes,[57] and they are linked if one gene has an up or down-regulatory genetic influence on the other, have small world network properties. Again, see Figure 2 for a scale-free network visualization. Now, it seems as though the cellular pathways of the brain may well be described as small-world networks compromising between maximum efficiency and lowest maintainance cost. This characterization will be useful to those testing hypotheses of brain organization and functions that are dependent upon knowledge of brain organization as it provides a modified view of brain modularity. While Fodor hypothesized that lower level units must operate efficiently, on fast biological timescales, and in relative isolation from other inputs and outputs from other parts of the brain, the network model will support some intuitions of this view, while offering a model of how deeply integrated various parts/modules of the brain are. This network model offers a way to test modular functional hypotheses based on a structural model of connection.

But the Connectome Project, as we have seen at the start of this essay (see Connectome Project: methods, aims, progress section), is hoping to correlate and link functional structure with physical structure. As we have seen, they have been moderately successful (putting aside, for the moment, the methodological worries that always accompany these kinds of studies that use various measurement tools) in correlating measurements of functional networks[58] with measurements of structural networks. Functional imaging (fMRI, EEG, DTI. MEG, and MEA) studies are showing an "encouraging degree of convergence"[59] in functional and structural networks.

Box 1[60]

- **Node:** is a vertex on the graphical representation of a network
- **Node degree:** number of connections that link it to the rest of the network. This is a fundamental measurement upon which the other measures are dependent
- **Clustering coefficient and motifs:** quantification of the number of connections between the nearest neighbors. Random networks have lower clustering. High clustering is associated with robustness because the network has a variety of paths to traverse to accomplish the same task.
- **Path length (efficiency):** minimum number of edges that must be traversed before one node can connect to another. Random networks have low average path lengths while regular lattices have long average path lengths (see Figure 5 for graphical representation of a path)
- **Connection density (cost):** $\frac{\text{number of edges}}{\text{total possible edges}}$
- **Hubs (centrality, robustness):** a hub is a node with a high node degree. Centrality measures how many of the shortest path lengths between every other node in the network passes through it. (Indicating centrality) Robustness refers to how well (integrated) the network can continue performing/functioning after a node deletion/lesion.
- **Modularity:** Estimated usually based on hierarchical clustering. There are provincial and connector hubs. (Local vs network connectors - a measurement of centrality) I would suggest that modularity, as observed from the meta-level is an emergent property of the network, much like a virtual governor. By analogy, I would suggest that such functional and structural modularity is at least partially similar to the way work groups within a company are partially or fully specialized in particular functions. While the individuals in that finance department may be able to perform many (if not all) of the functions of the individuals in the president's governance office, their particular constellation of factors, including their network of local connections, (a partial list of factors contributing to their efficiency could be generated) makes them particularly well suited to performing their functions as opposed to those functions of the president's office

Graph-analytical studies[61] have been used to study functional imaging data in both undirected and directed graphs. Directed graphs (often using Granger causality analysis) can be used to derive effective functionality measurements which can predict causal directionality in the graphs. However, most of the studies conducted thus far use undirected graph analysis which will just provide an overall measures of connectivity. These functional graph analysis studies have been showing common network structures to those measurements taken from DTI fiber tract-analysis[62] (a fiber structural measurement).[63] Measurements of global network efficiency, clustering, path length, and hubs were found to be similar to those found through structural analyses.

So, in relating structure to function, many questions remain to be resolved, many of which the connectome's findings will play a key informational role. We will need to understand how functional networks interact with the structure(s). We need specific details and measurements about how functional networks change over time- this can best be addressed by developmental studies. Do functional networks exist in a dynamically critical state at some or all frequency intervals?[64] What constraints are imposed anatomically and how does the long-term functional history of a network feed back on anatomical development? For all of these issues we will need to concretely nail down how the parameters of complex networks relate to cognitive and behavioral functions.[65]

In structural analysis of neural connectivity networks, three node motifs were found to be prevalent in the *C. Elegans*.[66] A preliminary conjecture about connectivity motifs would be that different motif classes support particular kinds of signaling.[67] This would make it seem as though particular network classes will be contributing to particular network functionalities. This has evolutionary origin importance and functional importance of specific motif classes.[68] Song *et al* have found much greater than expected likelihood of the occurrence of reciprocal connectivity between neurons.[69] Please refer to Figure 6 and Figure 7. In Figure 6, you will notice that some motifs have a much higher rate of occurrence than others. And some occur very seldom (motifs numbers 5, 8, 10, & 11) and one (7), conspicuously, has a very low rate of occurrence. This might lead one to wonder about the reasons why this might be, and it could lead to interesting hypotheses to test around why some motifs are not as structurally useful as others. Taking number 7 for example, the unidirectional circuit, would this lead to a jammed circuit signalling? Spiraling? Could this kind of motif causes some functions to become suceptible to have 'run away' effects without a stop mechanism? Is the relative lack of this functional circuit nature's equivalent of a stop function? Others, such as Prill have shown that "different structural motifs facilitate specific classes of behavior [such as] periodic, chaotic behavior or [are] promote[rs of] dynamic stability."[70] Still[71] further, others have shown that network motifs can exhibit different capacities for synchronization due to their conduction delays.[72] And "the high proportion of dual dyad motifs [...] has been linked to the capacity of such motifs to promote zero phase-lag synchrony across great spatial distances and hence long conduction delays in cortex."[73] The structural qualities that node motifs confer upon the network properties gives importance to the notion that structural questions will inform functional hypotheses.

Applications! Applications! Applications!

"Since Wernicke, Meynert and Dejerine it has been appreciated that many neurological and psychiatric disorders can be described as disconnectivity syndromes."[74] So we can use network properties as diagnostic markers of neurological conditions against appropriate comparison groups. These network property markers may also assist us in learning and hypothesizing about the kinds of functionalities that are deficient in such dysfunctions. Measurements could be clustering (or lack thereof) measures, reduced hierarchy, reduced centrality, fewer or more greater number of hubs, inefficient connections either by connection density or path lengths that are longer than the average. A couple of connectivity studies for Alzheimer's and Schizophrenia[75] have already been conducted and it seems probable that other disorders may be linked to connectivity abnormalities. Whether the abnormality is cause or a symptom of the abnormality would remain to be investigated, but such information would provide an important jumping-point for hypothesis generation and functional activity investigation.

Additionally, it has been found that many psychiatric conditions (or aspects) are heritable.[76] Given this fact, it's entirely possible that the kinds of factors that impact psychiatric conditions can be found as similar expressions in different, but genetically related, brains. "Measures of network topology may be worth investigating as intermediate phenotypes, or endophenotypes, that indicate the genetic risk for a neuropsychiatric disorder."[77]

Network analysis, as with the computational lesioning studies,[78] can be used to measure vulnerability and to classify different kinds of network vulnerabilities. Since it theoretically conceivable that different connections have different kinds of functionalities, it is possible that different kinds of networks, or hubs will have differing vulnerabilities according to the kinds of topological or dynamical advantages that the particular configuration has. "For example, scale-free networks are robust to random error but highly vulnerable to deletion of network hubs."[79] It is also entirely possible that network analysis can and will be used to understand how therapeutic treatments can affect network connectivity or how network connectivity can be used to measure how effective a treatment is.[80]

Appendix

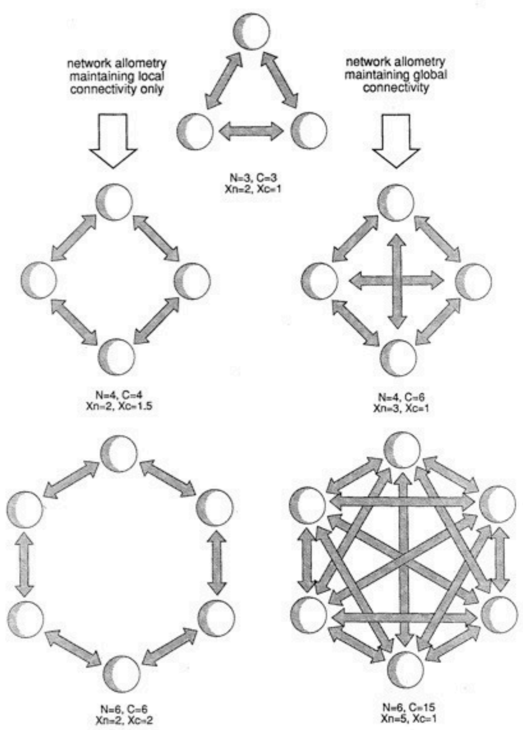


FIG. 7. The problem of network allometry is represented by the example of a very simple network (figure from Deacon, 1990*b*). A series of nodes (depicted as spheres) is connected reciprocally to each other (depicted by double arrows) in different size networks with different extremes of connectivity. Networks on the left exhibit low connectivity and those on the right exhibit maximum connectivity. N = total number of nodes; C = total number of reciprocal connections (note that in the nervous system reciprocal connections are separate connections); Xn = the number of other nodes to which any one node is directly connected; Xc = the mean number of connections intervening between any two arbitrary nodes. The growth of connections to nodes is a factorial function of the number of nodes in a fully connected network and a linear function of the number of nodes in a minimally connected network. Both low and high connectivity networks require major functional and structural trade-offs with size increase.

Figure 3 [81]

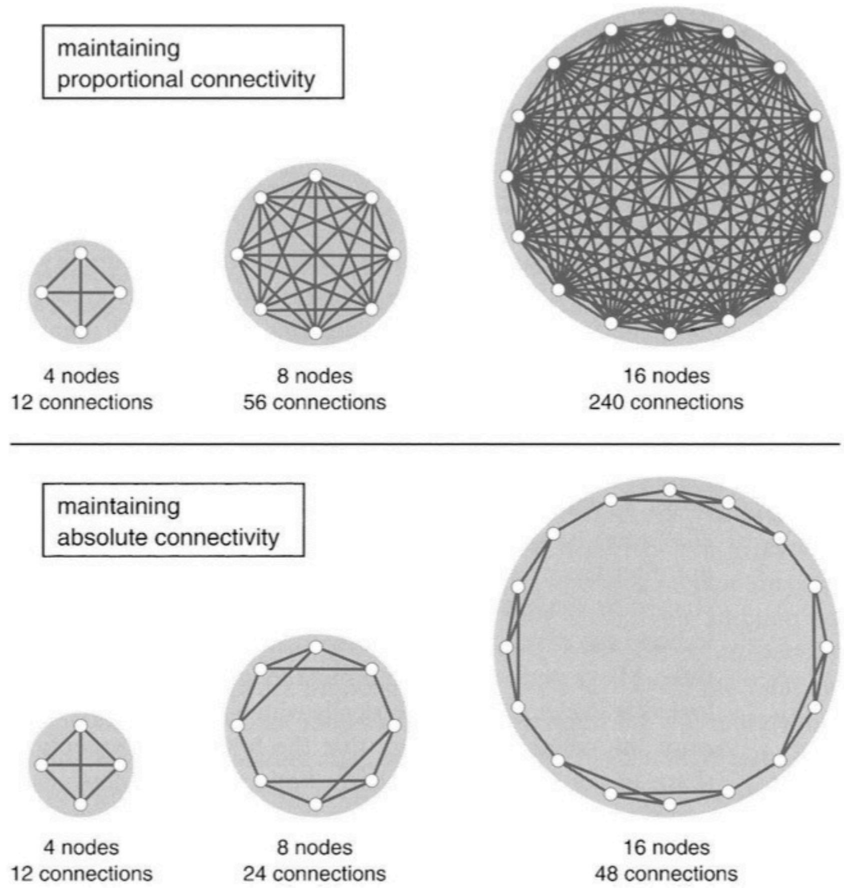
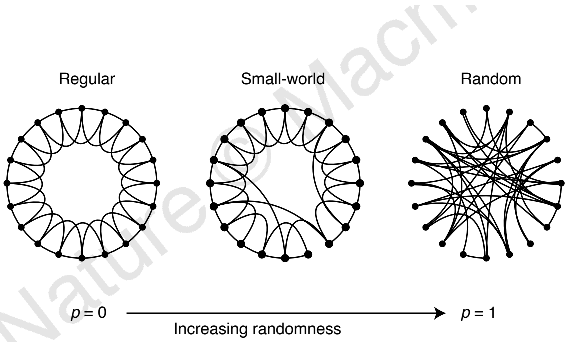


Figure 7.6
Network allometry and two models for scaling of connectivity. In both cases, network size increases from 4 to 8 to 16 nodes. If networks maintain "proportional connectivity" (top), the number of axons (and the wiring cost) rises exponentially. If networks maintain "absolute connectivity" (bottom), the number of axons increases linearly. Modified and redrawn after similar diagrams in Deacon (1990), Ringo (1991), and Striedter (2005).

Figure 4[82]



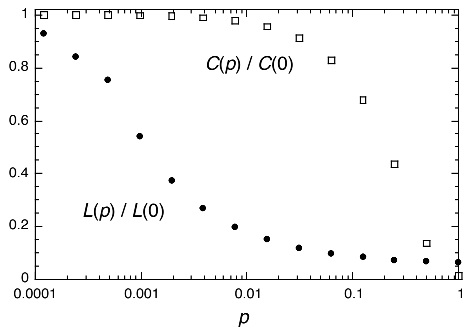


Figure 1 Random rewiring procedure for interpolating between a regular ring lattice and a random network, without altering the number of vertices or edges in the graph. We start with a ring of n vertices, each connected to its k nearest neighbours by undirected edges. (For clarity, $n = 20$ and $k = 4$ in the schematic examples shown here, but much larger n and k are used in the rest of this Letter.) We choose a vertex and the edge that connects it to its nearest neighbour in a clockwise sense. With probability p, we reconnect this edge to a vertex chosen uniformly at random over the entire ring, with duplicate edges forbidden; otherwise we leave the edge in place. We repeat this process by moving clockwise around the ring, considering each vertex in turn until one lap is completed. Next, we consider the edges that connect vertices to their second-nearest neighbours clockwise. As before, we randomly rewire each of these edges with probability p, and continue this process, circulating around the ring and proceeding outward to more distant neighbours after each lap, until each edge in the original lattice has been considered once. (As there are $nk/2$ edges in the entire graph, the rewiring process stops after $k/2$ laps.) Three realizations of this process are shown, for different values of p. For $p = 0$, the original ring is unchanged; as p increases, the graph becomes increasingly disordered until for $p = 1$, all edges are rewired randomly. One of our main results is that for intermediate values of p, the graph is a small-world network: highly clustered like a regular graph, yet with small characteristic path length, like a random graph. (See Fig. 2.)

Figure 2 Characteristic path length $L(p)$ and clustering coefficient $C(p)$ for t family of randomly rewired graphs described in Fig. 1. Here L is defined as t number of edges in the shortest path between two vertices, averaged over pairs of vertices. The clustering coefficient $C(p)$ is defined as follows. Suppo that a vertex v has k_v neighbours; then at most $k_v(k_v - 1)/2$ edges can ex between them (this occurs when every neighbour of v is connected to every oth neighbour of v). Let C_v denote the fraction of these allowable edges that actua exist. Define C as the average of C_v over all v. For friendship networks, the statistics have intuitive meanings: L is the average number of friendships in t shortest chain connecting two people; C_v reflects the extent to which friends c are also friends of each other; and thus C measures the cliquishness of a typi friendship circle. The data shown in the figure are averages over 20 rand realizations of the rewiring process described in Fig. 1, and have been normaliz by the values $L(0)$, $C(0)$ for a regular lattice. All the graphs have $n = 1{,}000$ vertic and an average degree of $k = 10$ edges per vertex. We note that a logarithn horizontal scale has been used to resolve the rapid drop in $L(p)$, corresponding the onset of the small-world phenomenon. During this drop, $C(p)$ remains alm constant at its value for the regular lattice, indicating that the transition to a sm world is almost undetectable at the local level.

Figure 1[83]

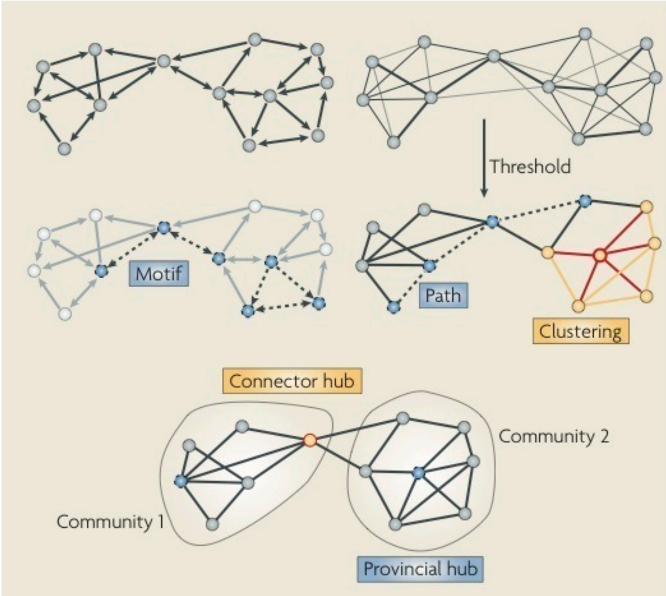


Figure 5[84]

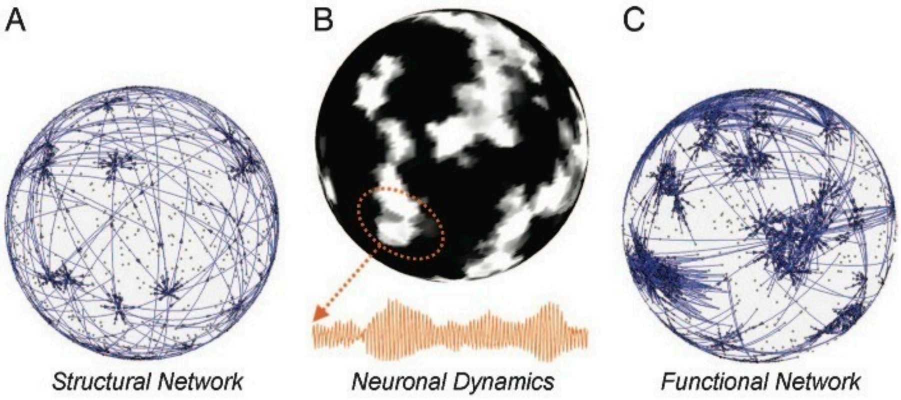
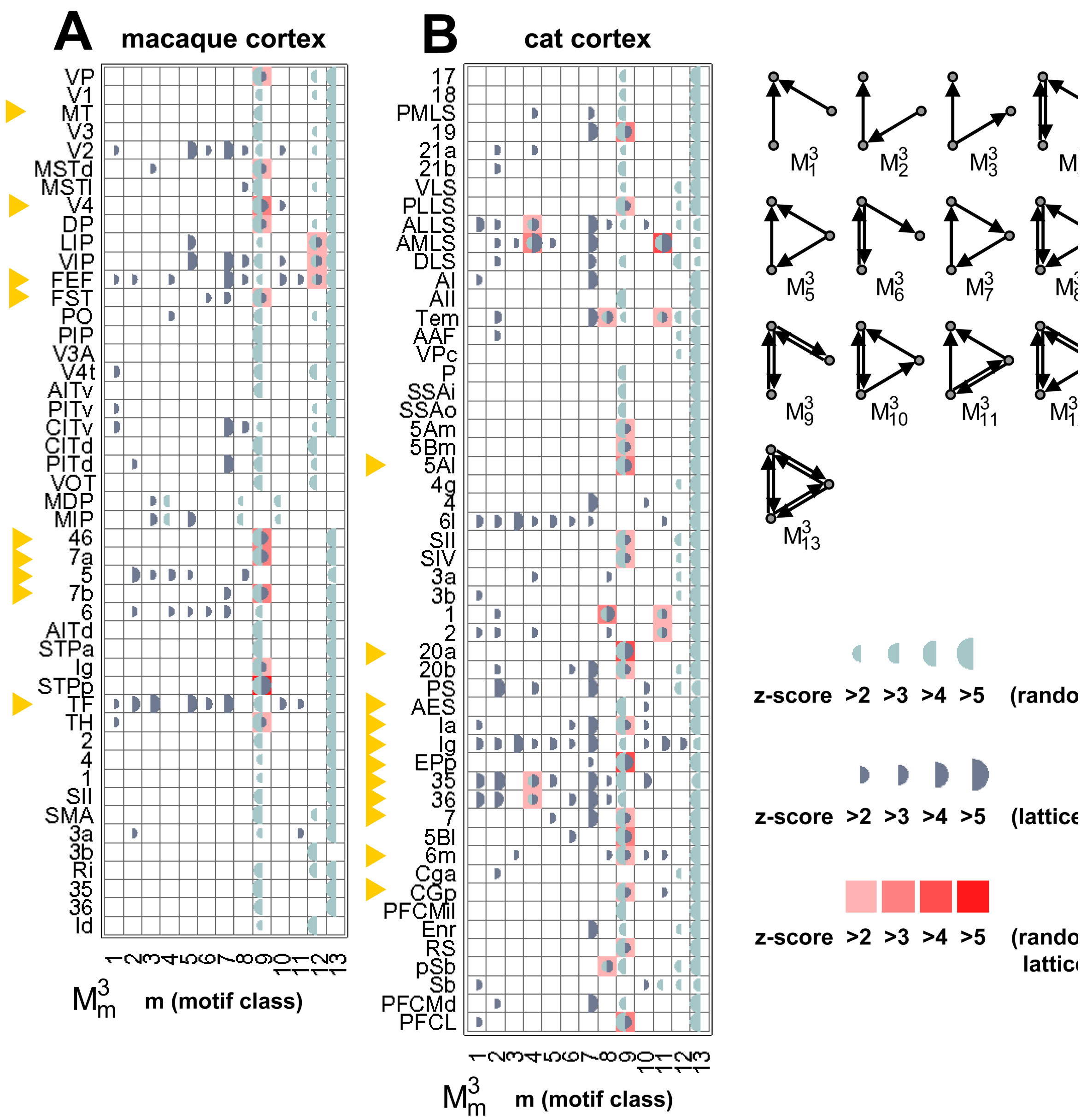


Fig. 1. Relationship of structural to functional connectivity networks. We built a demonstration model consisting of a set of 1,600 modeled neural mean field units arranged on a sphere and engaging in noise-driven spontaneous activity. (*A*) The anatomical connection pattern, shown only for a few randomly selected neural units, consists of a mix of mostly local (clustered) connections and a few connections made over longer distances. (*B*) A snapshot and an EEG-like recording trace of the dynamical neuronal activity pattern. Neuronal dynamics is characterized by complex spatial and temporal structure across multiple scales [supporting information (SI) Movie 1]. (*C*) A functional connectivity network obtained from a thresholded correlation matrix calculated from the dynamics shown in *B*. In this example, both structural and functional connectivity patterns exhibit small-world attributes.

Figure 8[85]



A
macaque cortex
VP
V1
MT
V3
V2
MSTd
MSTl
V4
DP
LIP
VIP
FEF
FST
PO
PIP
V3A
V4t
AITv
PITv
CITv
CITd
PITd
VOT
MDP
MIP
46
7a
5
7b
6
AITd
STPa
Ig
STPp
TF
TH
2
4
1
SII
SMA
3a
3b
Ri
35
36
Id
m (motif class)
B
cat cortex
17
18
PMLS
19
21a
21b
VLS
PLLS
ALLS
AMLS
DLS
AI
AII
Tem
AAF
VPc
P
SSAi
SSAo
5Am
5Bm
5Al
4g
4
6l
SII
SIV
3a
3b
1
2
20a
20b
PS
AES
Ia
Ig
EPp
35
36
7
5Bl
6m
Cga
CGp
PFCMil
Enr
RS
pSb
Sb
PFCMd
PFCL
m (motif class)
z-score >2 >3 >4 >5

Figure 7[86]

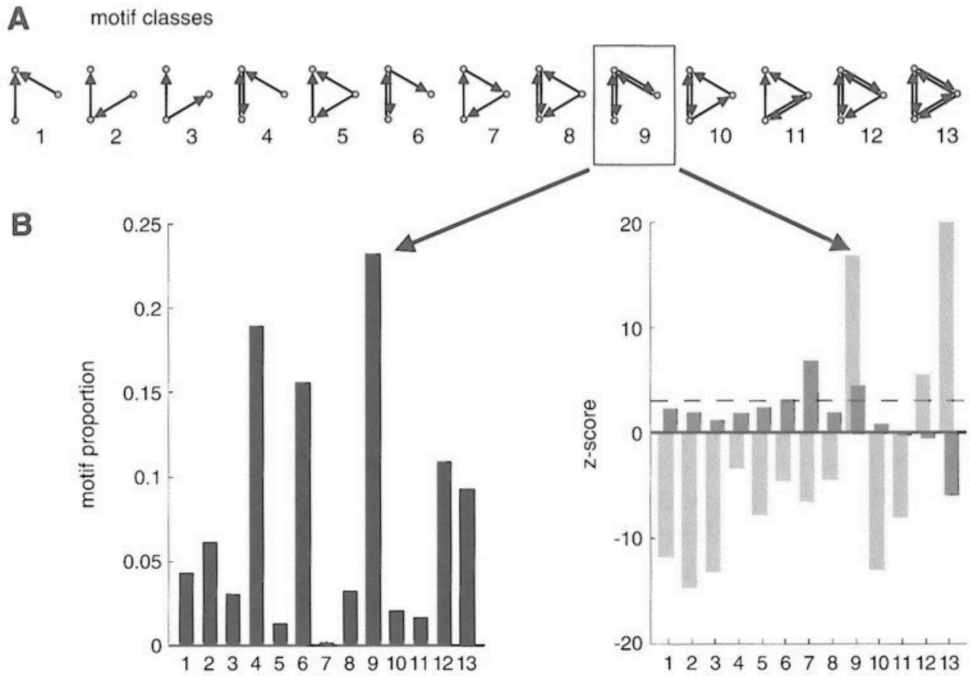


Figure 6.4
Motifs in macaque cortex. (A) All 13 possible motif classes for three nodes linked by directed edges. (B) Motif frequency distribution for a structural connection matrix of macaque cortex containing 47 nodes and 505 directed edges (see figure 2.6). Motif counts are compared (right panel) against populations of random (light gray) and lattice networks (dark gray). Only one motif class (motif class 9, referred to as the "dual dyad" motif) is found in significantly increased proportion relative to both random and lattice controls. Data were redrawn from Sporns et al. (2007).

Figure 6[87]

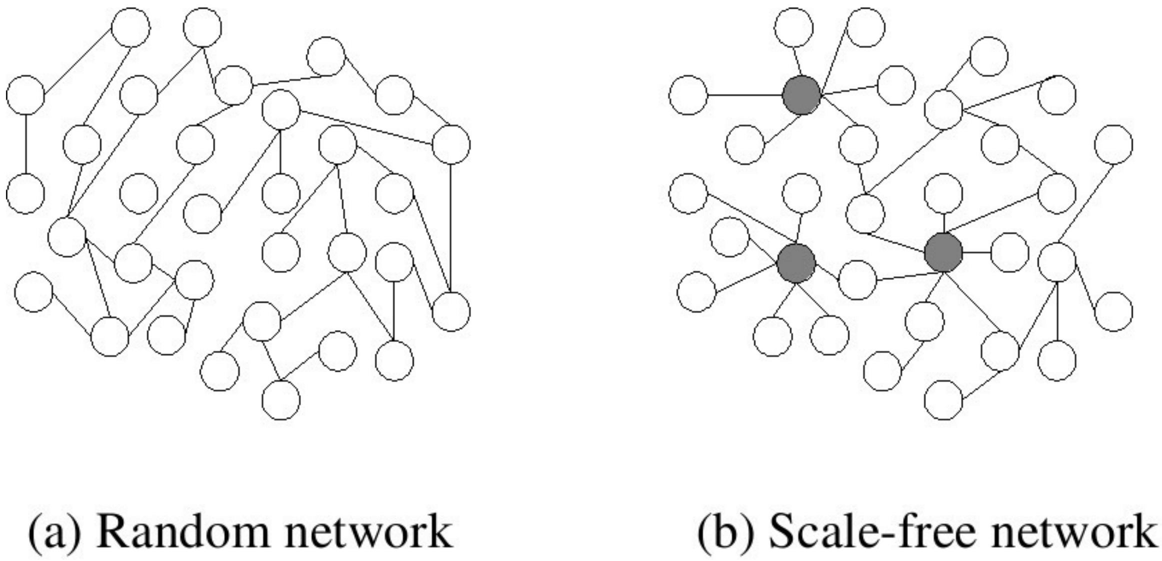


Figure 2 [88]

Examples of a random network and a scale-free network. Each graph has 32 nodes and 32 links. Note that both were chosen to be connected and to look nice on the plane, so they are not entirely random.

Notes

1. {Churchland, 1989 #1553@p. 378}
2. {Cajal, 1995 #1609}
3. {Crick, 1993 #1610}
4. {Jahanshad, 2010 #1537}
5. {Bullmore, 2009 #1555@p. 189}
6. {Bullmore, 2009 #1555@p. 193}
7. {Tononi, 1994 #1567}
8. {Bullmore, 2009 #1555@p. 186}
9. {Bullmore, 2009 #1555@p. 192}{Grutzendler, 2002 #1579} {Alvarez, 2007 #1578}
10. {Bullmore, 2009 #1555@p. 192}
11. {Morcom, 2007 #1541}
12. {Bullmore, 2009 #1555@p. 192-193}
13. {Siri, 2007 #1583}{Bullmore, 2009 #1555@p. 193}
14. {Song, 2005 #1611}
15. {Bullmore, 2009 #1555@p. 190}{Hagmann, 2008 #1548}
16. {Bullmore, 2009 #1555@p. 190}
17. {Prill, 2005 #1602}
18. {Drachman, 2005 #1549}
19. {Kosinski, 2007 #1550}
20. National Institutes of Health
21. {Morgan, 1999 #1551@p.17}
22. {Morgan, 1999 #1551@p.10}
23. {Morgan, 1999 #1551@p.18-19}
24. {Churchland, 1989 #1553@p. 324-325}
25. {Churchland, 1989 #1553@p. 334}
26. {Churchland, 1994 #1529}
27. {Bermúdez, 2005 #1613@p. 38}
28. {Churchland, 1989 #1553@p. 349}
29. {Churchland, 1989 #1553@p. 351}
30. {Churchland, 1989 #1553@p. 349}
31. {Churchland, 1989 #1553@p. 360}
32. {Churchland, 1989 #1553@p. 360}
33. {Churchland, 1989 #1553@p. 358}
34. {Churchland, 1989 #1553@p. 396}
35. {Churchland, 1989 #1553@p. 396}
36. {Churchland, 1989 #1553@p. 356}

37. {Churchland, 1989 #1553@p. 359}
38. {Churchland, 1989 #1553@p. 360-361 (emphasis mine)}
39. {Churchland, 1989 #1553@p. 398}
40. {Smit, 2008 #1589} {Posthuma, 2005 #1590}
41. {Morcom, 2007 #1541}
42. {Churchland, 1989 #1553@p. 373}
43. {Churchland, 1989 #1553@p.365}{Dewan, 1976 #1552}
44. {Churchland, 1989 #1553@p. 365}
45. "some types of mental state… will turn out to be "virtual governors…The theoretical future of a unified folk psychology category, "memory," or "learning," looks bleak because the domain of input-output phenomena is itself undergoing re-description and reclassification. And the interconnected set of categories "consciousness," "awareness," and "attention" are themselves evolving and may in time be transformed rather radically." {Churchland, 1989 #1553 @p. 368}
46. {Churchland, 1989 #1553@p. 367}
47. {Churchland, 1989 #1553@p. 368-369}
48. {Churchland, 1989 #1553@p. 369}
49. {Churchland, 1989 #1553@p. 374}
50. {Bermúdez, 2005 #1613@p. 31}
51. {Bermúdez, 2005 #1613@p. 31}
52. {Sporns, 2010 #1554@p. 2}
53. {Achard, 2007 #1568}
54. {Sporns, 2000 #1566}
55. {Jeong, 2000 #1560}
56. {Bork, 2004 #1558}
57. {Van Noort, 2004 #1559}
58. {Eguiluz, 2005 #1575}
59. {Bullmore, 2009 #1555@p. 190}
60. adapted from {Bullmore, 2009 #1555}
61. {McIntosh, 1994 #1570;Achard, 2006 #267;Brovelli, 2004 #1573;Bullmore, 2000 #1571;Ferrarini, 2009 #1576;Friston, 2003 #1572;Meunier, 2009 #1577;Salvador, 2005 #1574}
62. http://www.slideshare.net/kitware/a-quantitative-dti-fiber-tract-analysis-suite898
63. {Bullmore, 2009 #1555@p. 190}
64. {Barahona, 2002 #1606;Kitzbichler, 2009 #1608;Shin, 2006 #1607}
65. {Bullmore, 2009 #1555@p. 196}
66. {White, 1985 #1600}
67. {Zhigulin, 2004 #1601}
68. {Sporns, 2010 #1554@p. 107}
69. {Sporns, 2010 #1554@p. 108}
70. {Prill, 2005 #1602}
71. {Sporns, 2010 #1554@p. 108}
72. {Sporns, 2010 #1554@p. 109} {D'Huys, 2008 #1603}

73. {Sporns, 2010 #1554@p. 109}{Vicente, 2008 #1605}
74. {Bullmore, 2009 #1555@p. 193}
75. {Liu, 2008 #1586;Micheloyannis, 2006 #1587}
76. {Schmitt, 2008 #1591;Smit, 2008 #1589;Sporns, 2006 #216}
77. {Bullmore, 2009 #1555@p. 195}
78. {Achard, 2006 #267;Honey, 2008 #1595;Kaiser, 2007 #1594}
79. {Bullmore, 2009 #1555@p. 195}
80. {Bullmore, 2009 #1555@p. 195-196}{Achard, 2007 #1568}{Honey, 2003 #1596}{Schwarz, 2007 #1597}{Stoffers, 2008 #1598}
81. {Deacon, 1990 #1538}
82. {Sporns, 2010 #1554@p.144}
83. {Watts, 1998 #1546@p. 441}
84. {Bullmore, 2009 #1555}
85. {Sporns, 2006 #1556}
86. {Sporns, 2007 #265}
87. {Sporns, 2010 #1554@p.109}
88. {Castillo, 2004 #1557}

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